Responses to Reviewers' comments

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title: The ocean's biological and preformed carbon pumps in perpetually slower and warmer oceans
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Response to Reviewer 1

Note: Reviewer 1 comments are indicated in black and our responses are in blue.

The authors improved the manuscript and answered to the comments and questions of the reviewers. I recommend publication in Biogeosciences after addressing the minor comments below. The line numbers correspond to the ones in the revised manuscript with track changes.

We thank Reviewer 1 for the positive feedback and for recommending our manuscript for publication in Biogeosciences. The minor comments are addressed below.

I understand that ACCESS1.3 is state-of-the art climate model and that previous publication optimized the PCO2 biogeochemistry model embedded in the ACCESS1.3 circulation. The choice of this model is clearly justified for this study. Regarding the influence of meltwater input from terrestrial ice sheets on the ventilation and storage of carbon in the deep ocean, the authors added in the discussion of the revised manuscript that they expect an exacerbated reduction in future Southern Ocean ventilation from the ice-sheet meltwater. The future influence of meltwater, and especially Antarctic meltwater, on regional and global climate under RCP4.5 or RCP8.5 scenarios is likely to result in strong Southern Ocean changes. The authors mentioned the ACCESS-ESM1.5 model that was used in Purich and England (2023) to investigate the effect of ice-sheet meltwater in the Southern Ocean. ACCESS-ESM1.5 is based on ACCESS1.3, so I wonder if it would have been possible to investigate the effect of ice-sheet meltwater using this model instead of ACCESS1.3? Or otherwise via sensitivity experiments?

We agree that the influence of ice-sheet meltwater will be very important in the future, especially in the Southern Ocean. Given that ACCESS-ESM1.5 is essentially an updated version of ACCESS1.3, it is appropriate to discuss the experiments by Purich and England (2023) in our Discussion section, and we think this is sufficient to highlight the issue. Repeating our analyses for a circulation from ACCESS-ESM1.5 (with and without ice-sheet melt-water effects) is well out of scope here as it would require constructing the appropriate transport matrix (including tuning mixing and validating transport), not to mention additional analysis and interpretation in the context of our frozen-in-time steady-state framework. (No changes to the manuscript in response to this comment.)

Section 3.1.2 discusses how warming stimulates euphotic POC respiration and affects particle sinking rates. This section could provide a clearer distinction between the direct and indirect effects of warming on these processes.

We agree that our discussion of these effects could have been clearer. In response, we have revised the last paragraph of Section 3.1.2 to start with:

> In PCO2, warming has opposing effects on the key mechanisms controlling export efficiency. On one hand, warming stimulates respiration of POC in the euphotic zone leaving less POC to be exported, which tends to decrease export ratios. On the other hand, warming reduces water viscosity so that particles sink faster out of the euphotic zone, which tends to increase export ratios (see Eqs. (2) and (A3) in Pasquier et al., 2023).

L210: please detail the change in C:P uptake ratio

C:P uptake ratio increases when phosphate concentration decreases in the parameterization of Eq. (2) that we adopted from Galbraith and Martiny (2015). In response, we have added a reference to Eq. (2), and the revised sentence now reads:

> The decreased nutrient supply (i.e., reduced phosphate concentration) reduces biological carbon uptake despite increasing the C:P uptake ratio as parameterized by Eq. (2) following Galbraith and Martiny (2015).

L306: relative to complete surface saturation

Thanks for catching this typo. We have corrected "realtive".

L386: Note that if we had

We agree that the suggested wording is better. We have replaced "Note that, had we" with "Note that if we had".

Response to Reviewer 2

Note: Reviewer 2 comments are indicated in black and our responses are in blue.

The authors have addressed my previous comments in their answer and the new version of the manuscript is now much improved. In their revision, the authors have clarified the methodology and expanded the discussion of their results. There is just a minor point regarding the timescales considered in this study which I think should be addressed. The authors state that their steady state study captures the response of the system on all timescales. I think it could lead to confusion (as discussed by the authors, some timescales are not taken into account in the biogeochemical simulations, for example seasonal timescales) and I suggest clarifying this point in the manuscript. Besides, I have noticed a few additional minor and technical comments on specific sections of the text.

Overall, the paper has novel aspects with a partitioning of preformed and regenerated dissolved inorganic carbon in accounting for the source and sink processes and it makes a valuable contribution to the understanding of carbon pump and its changes under two climate change scenarios. I look forward to seeing the final manuscript published after these final issues are addressed.

We thank Reviewer 2 for the positive feedback and for the additional comments.

A distinction must be made between the timescales of variability (e.g., seasonal) and the timescales of the steady-state system (e.g., decadal thermocline ventilation and centennial deep ventilation). We agree that our framework does not capture variability on any timescale, as we focus on steady-state biogeochemistry in frozen-in-time circulations. However, our model does capture the response of the system to changes in decadal-mean ocean state (preindustrial to perpetual 2090s) integrated over all the timescales of the system. We have clarified this distinction throughout (including in the Abstract). Our point-by-point responses to the minor comments follow below.

Please see all my detailed and technical comments below.

L. 4-6: "Focusing on steady-state changes from preindustrial conditions allows us to capture the response of the system on all timescales, not just on the sub-centennial timescales of typical transient simulations." I don't think it allows capturing the response of the system on all timescales and I suggest this sentence needs revising.

To clarify that our analysis captures the system's response integrated over all timescales, we have revised this passage to read:

> Focusing on steady-state changes from preindustrial conditions allows us to capture the response of the system integrated over all the timescales of the steady-state biogeochemistry, as opposed to typical transient simulations that capture only sub-centennial timescales.

L. 51: I suggest removing they probe all the timescales of the system and.

While our frozen-in-time steady-state framework cannot capture natural variability on any timescale, we do capture the system's response to change in ocean state integrated over all timescales as discussed above. In response, we have revised the passage as follows:

> Probing the system in steady state is advantageous because it avoids the complications of transience by integrating the system's response to change in perpetual decadal-mean ocean state on all timescales.

L. 74: change analysed to analyzed, as used throughout the manuscript.

Thanks for catching this. Done.

L. 94: I suggest rephrasing this sentence as for example: Instead, our steady-state solutions allow us to determine what the asymptotically long-term

We have rephrased as suggested.

L. 141: I suggest defining r

Thank you for the suggestion. The revised sentence now reads:

> Note that in our model, carbon can only enter or exit the ocean through air–sea exchange so that in steady-state equilibrium there is no net carbon source or sink when globally integrating over all locations $\boldsymbol{r}$, i.e., $\int J_{\text{atm}}(\boldsymbol{r})\,\mathrm{d}^3\boldsymbol{r} = 0$.

L. 219-220: In response to a previous comment, the authors have added here a sentence in which they refer to Fig. D3. The contributions from each export pathway shown on this figure are described in the next sub-sections. I suggest moving this sentence in one of the next sub-sections and here referring to this next sub-section, or, at least, guiding the reading of this figure, by indicating, for example, the relevant contributions (and the colour of the corresponding curves).

We respectfully disagree that this suggestion would improve readability, with the order of presentation of the various aspects of export is somewhat arbitrary. While the contributions from each export pathway to global export and regenerated C inventories are described later (Fig. 5), the colored curves in Fig. D3 show the latitudinal decomposition of ΔJ_{ex} for each export pathway into sub-contributions from changes in uptake and changes in export ratios, i.e., from each of $f\Delta U$, $U\Delta f$, and $\Delta f \Delta U$. In addition, the globally integrated ΔJ_{ex} from each pathway and for each scenario were already shown in the panels of Figure D3 as indicated in its caption. Adding the $4 \times 8 = 32$ values of the globally-integrated sub-contributions to the text or the figure would make them very cluttered for little additional insight. (No change to the manuscript in response to this comment.)

L. 559-562: I suggest rephrasing the last part of the sentence.

Agreed. The sentence now reads:

> While the responses of the biological and preformed pumps are driven by different mechanisms with widely different response timescales, the regenerated and preformed DIC inventories both increase by similar absolute amounts when the contributions from the longest response timescales are captured in steady state.

Additional note from the authors

As emphasized in the previous round of revisions, our idealized framework of steady-state biogeochemistry embedded in frozen-in-time ocean states precludes the interpretation of our results as predictions of the future. During the review of a separate manuscript on ocean deoxygenation using the same framework, a reviewer made us aware that our use of the term "future" could be confusing in this regard. We have therefore clarified in this revision that our results quantify the carbon pumps in perpetually warmer and slower oceans. Specifically, we have replaced "future" with "warmer and slower" or "perpetual 2090s" throughout the manuscript, including title, abstract, and figures. We have also revised the first caveat in the Discussion section to read as follows:

> Our results for steady-state biogeochemistry embedded in frozen-in-time ocean states are not predictions of the future. The real ocean will keep changing for many centuries beyond the 2090s and its future long-term dynamical equilibrium (if forcing ever stabilizes) will likely be vastly different from the ocean states analyzed here (e.g., Schmittner et al., 2008). However, our idealized steady states do reveal the key mechanisms driving the system's responses to future change.

We think these revisions will prevent potential confusion and help readers to better understand the nature of our analysis and the implications of our results.

References

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